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A LESSON IN SYSTEMATIC BRYOLOGY.

DR. GEORGE N. BEST.

[Read at the meeting of the Sullivant Moss Chapter, Philadelpha, Pa., Dec. 31, 1904.]

While engaged in the study of some specimens of *Thuidium abietinum*, collected in Minnesota by Prof. J. M. Holzinger, I noticed that one of these differed from the ordinary forms of this species in that the leaves were somewhat larger, longer and more gradually acuminate, more strongly falcate-secund, and that the leaf cells were narrower and more elongated. Recognizing in these variations, at least in part, the characters on which Mitten had based his *Thuidium hystricosum*, and having an authentic specimen of this species at hand, a comparison was made, with the result of finding them nearly identical. The leaves, however, of *Thuidium hystricosum* were slighly larger and the leaf cells somewhat longer, but the differences in these respects were hardly appreciable. Not being fully satisfied that the Minnesota moss was indeed *Thuidium hystricosum*, I submitted it to Dr. Mitten, who considered it a form of *Thuidium abietinum*. Not comprehending how it was that two mosses, so nearly identical, should be referred to different species, I decided on an appeal, and in this instance to my herbarium, which contained about forty specimens of *Thuidium abietinum* from an extended range; that is to say, from Germany, Switzerland, France, Belgium, England, Labrador, various localities in the United States and in Canada, and as far north as the Yukon Territory. With few execeptions these specimens had been determined by well known bryologists. It may therefore be confidently assumed that they were correctly named. In fact, while *Thuidium abietinum* is but very rarely found in fruit, its specific characters are so well marked that no difficulty need be experienced in distinguishing it from all of the North American and European Thuidia, unless it be from *Thuidium hystricosum*, which I regard as simply a form of this species.

In my examination of these specimens attention was more especially directed to

1. The size and shape of the stem leaves taken from the middle third of the stem.
2. The size and shape of the leaf cells of these same leaves.
3. The presence or absence of a central strand in the stems.

As a tabulated statement of the results obtained from the examination of each of these specimens would be somewhat confusing, a few representative specimens have been selected by which it will be seen that we have here a series of forms, intergrading and ascending, and that the space between the extremes, that is between the lowest and the highest forms, is so covered by intermediate forms as to leave no doubt as to their being but variants of one specific type.

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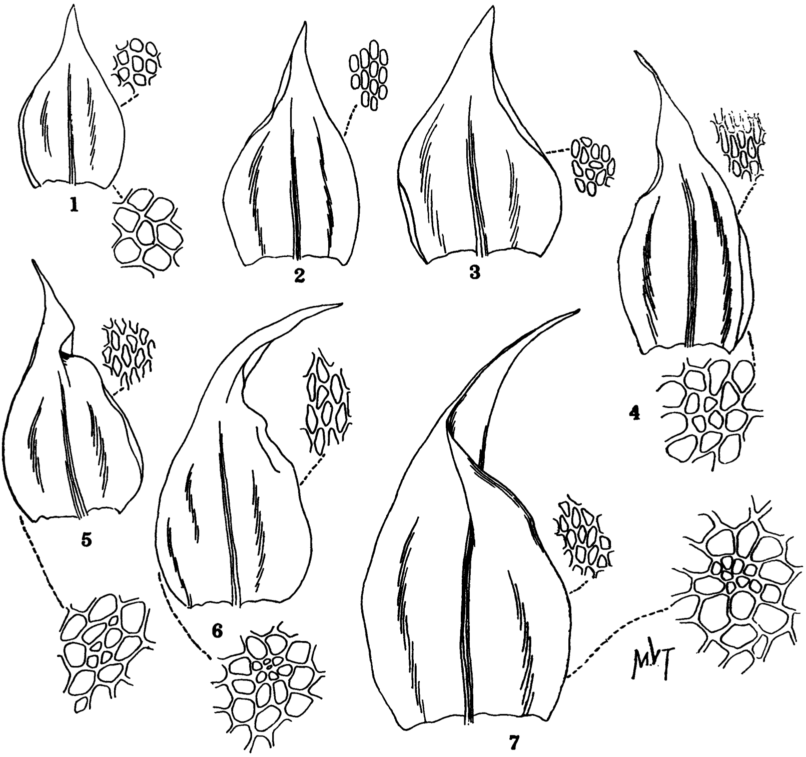

PLATE II. Thuidium—Leaves, median cells and stem section.

Fig. 1. Specimen from Crawford's Notch, New Hampshire; central strand none; leaves .8 mm. long, .5 mm. wide, ovate, gradually acute to abruptly acuminate, acumen straight or slighly curved; leaf cells somewhat uniform; median broadly oval to roundish oval, nearly isodiametric.

Fig. 2. Specimen from Northeastern Minnesota; central strand none; leaves differ from those of the preceding in being slightly larger, 1 mm. long, .6 mm. wide, more curved and longer acuminate: leaf cells larger and orbicular.

Fig. 3. Specimen from Labrador; central strand none; leaves broadly ovate, 1 mm. long, .8 mm. wide, gradually acute to abruptly acuminate, acumen curved; leaf cells somewhat irregular; median rhombic to oval-oblong.

Fig. 4. Specimen from Germany; central strand rudimentary; leaves 1.2 mm. long, .8 mm. wide: differ but little from those of the preceding: leaf cells somewhat longer and narrower, oval-rhombic to fusiform.

Fig. 5. Specimen from Niagara Falls: central strand rudimentary; leaves ovate-lanceolate, 1.4 mm. long, .8 mm. wide, slightly curved, narrowly acuminate; median leaf cells irregularly fusiform with subsinuate margins; apical cells linear-fusiform.

Fig. 6. Specimen from Northern Minnesota; central strand present but small and indistinct; leaves ovate-lanceolate, 1.6 mm. long, .9 mm. wide, narrowly acuminate, strongly curved: median leaf cells irregular, oval-rhombic to oblong-fusiform: apical linear-fusiform.

Fig. 7. Specimen from North Downs, England; central strand small, distinct; leaves ovate-lanceolate, 1.8 mm. long, .9 mm. wide, falcate-secund, long and narrowly acuminate; leaf cells not uniform, median oval-rhombic to fusiform; apical long linear-fusiform.—*Thuidium hystricosum* from Dr. Mitten.

To recapitulate: The leaves of the lowest form are ovate, .8 mm. long, .5 mm. wide, gradually acute to short acuminate, acumen straight or slightly curved, In the highest form the leaves are ovate-lanceolate, 1.8 mm. long, .9 mm. wide, falcate-secund, long and narrowly acuminate. The leaf cells in the lowest form are broadly oval, nearly isodiametric; in the highest form the leaf cells are irregular, the median oval-rhombic to fusiform and the apical linear-fusiform. In the lowest form the stems are without a central strand; in the highest form the central strand is small but distinct. In the intermediate forms the central strand is absent or rudimentary, and the median leaf cells pass from oval-rhombic to rhombic-fusiform or oblong-fusiform.

The parts of the plants which exhibit a fair degree of constancy or fixedness are the multiform paraphyllia, the spreading, pinnate, tapering branches, two-rowed on either side of the oval stem; the reddish, incrassate, porose basal cells of the stem leaves, the central papillæ on each surface of the leaf cells, longer on the lower than on the upper, and the midribs about three-fourths the length of the biplicate leaves.

The series here presented is one of the simplest of its kind and illustrates quite well the evolution of a specific type. Its individuals being usually sterile and its propagation effected almost wholly by vegetative means, the unknown factor of hybridism is thereby practically eliminated. While it would be useless to deny the possibility of still lower and still higher forms appearing, yet, so far as this series goes, it is without a break or a missing link in the evolutionary chain. Parts of series, broken series, the so-called groups, are always in evidence, but a series in which the connection of intergrading forms is so well kept up as in the one before us, is not so common. This difficulty however is probably to be attributed more to a scarcity of material, at least in many cases, than to an absence of such connecting forms in nature.

It is generally assumed that the lowest forms of a series are the oldest, and therefore properly represent the specific type, and that the highest forms are the latest and mark the evolution of this type. While this proposition is probably well founded, it scarcely admits of a demonstration: and while it is reasonably certain that plants appear in series, it is an open question whether these series are always ascending: possibly some are ascending, some comparatively stationary and others descending, in line for final extinction.

It is however more for its practical application in systematic work than for any theoretical consideration that this series is presented at this time, as I am fully convinced that if we are to make any considerable progress in systematic bryology, it must be along these lines. The younger bryologists among us know quite well what difficulties they have to contend with in determining the mosses they collect. If they succeed in tracing, with a reasonable degree of certainty, a given specimen to its genus, they are not infrequently baffled, notwithstanding the most persistent efforts, in making a satisfactory reference as to the species, two or three descriptions agreeing equally as well as any one. By not a little hard work and many bitter failures, the older ones among us have learned to discount these descriptions to their actual value, and are thereby enabled to approximate a determination with a reasonable degree of confidence.

In every system of classification, dealing with plants, the species is the unit of aggregation. It is therefore of the utmost importance that we should have a definite conception of what a species really is. In botany there is probably nothing so unscientific as the looseness with which species are usually made. Huxley defines a species as "the smallest group to which distinctive and invariable characters can be assigned." This is the traditional species, originating in a special act of creation, (whatever that may mean,) and continuing through all time with but limited variation, sexual trespass among the individuals of which being punished with annihilation. Huxley's species may be both logical and theological, but it is objectionable for the reason that it is too restrictive. Species with invariable characters are rare and probably do not exist outside of a single individual. Alphonse de Candolle says: "They are mistaken who repeat that the greater part of our species are clearly limited and that the doubtful species are in a feeble minority. This seemed to be true so long as a genus was imperfectly known and its species were founded on a few specimens, that is to say, were provisional. Just as we come to know them better intermediate forms flow in and doubts as to specific limits augment."

In this connection it may be well to quote Darwin in the chapter on "Variation under Nature" in *Origin of Species.* "From these remarks," he says, "it will be seen that I look at the term species as one arbitrarily given, for the sake of convenience, to a set of individuals closely resembling each other and that it does not essentially differ from the term variety which is given to less distinct and more fluctuating forms." It is needless to say that Darwin's pet theory was "natural selection." Before him other scientists had shown that in the organic world the indications pointed to an evolutionary process. Darwin claimed that the "survival of the fittest" was the keynote to this process. To better cope with the changing conditions continually presenting themselves in the world, species gave off varieties, these either disappeared in the course of time or became species, and these in turn gave off other varieties which likewise either disappeared or became species, and so on and on.

Under the guidance of such teachings it is no great wonder that the output of new species is as large as it is, for if Huxley's views be followed, calling for not only distinctive but invariable characters, and these not rarely the lowest in the scale of taxonomic values, the number of species must of necessity be largely augmented. On the other hand, if species is a term arbitrarily given for convenience sake to a set of individuals closely resembling each other, as Darwin would have us believe, the easiest way to dispose of a given specimen which does not readily fall in the line of recognized species, is to dub it a "new species" and let it conveniently pass at that. Thus it happens that mere scraps, without sexual organs or fruit, gathered from the "four corners," serve as the material out of which large batches of new species are made apparently more for the glory of the makers than for the advancement of science. Taking advantage of Darwin's "convenience" for exploiting their ephemeral creations, they seem to lose sight of one of his requirements, in fact the principal one, namely, a "set of individuals." Possibly it is assumed that this "set" is found in a single specimen—I refer to mosses—but I am inclined to think that this assumption is a contravention of his real meaning. By "set" he probably intends to include a number of individuals not appearing in one tuft or from a single locality.

But Huxley's "group" and Darwin's "set" are alike objectionable in that they imply something artificial. In fact this is the dangerous reef upon which we are now stranded. Is it not about time for us to break away from these Linnean conceptions and to settle ourselves down to a more rational basis? Evolution does its work along well defined lines, not sporadically. If a species means anything, it means a series of individuals possessing certain distinctive but not invariable characters. In the higher orders of plants the sexual organs furnish excellent characters, not only for generic, but for specific distinctions. In the *Musci* the sexual organs are not rarely absent, and when present are not so distinctive. We must therefore rely on other parts for the differentiation of species. The leaves, and especially the leaf cells, are supposed to furnish valuable characters. We have seen, however, in the series we have examined that these were more or less variable, and that this variability depended on individual differences. The characters derived from the size and shape of the leaves and the size and shape of the leaf cells, are to be regarded as complementary to those more constant, and are not of themselves to be considered specific. In the species of some genera in which this variability is not so marked, the leaves and the leaf cells possess a higher degree of taxonomic value.

In his excellent work, *Die Laubmoose*, Limpricht places considerable stress upon certain anatomical characters derived from transverse sections of the stems and midribs. Whether these will prove more reliable than those more commonly employed or be more acceptable to bryologists in general is as yet an open question. Judging from my own observations I have no hesitation in saying that while undoubtedly valuable they vary more than would naturally be supposed, this becoming apparent when the extremes of growth are compared, and are therefore to be taken with due allowance. It cannot however be too strongly urged that in the delimitation of species the whole plant, and not a single part of it to the exclusion of the other parts, should be the subject of investigation.

From these considerations the fallacy of requiring each individual to conform in all its parts to the original type of a given species becomes evident. This type is just as likely to be one of the lower or one of the higher forms of the species in question, as it is to be an intermediate form. In either case exact duplicates should not be demanded. Mosses do not grow so much after a mathematical formula as some bryologists would have us believe.

It also appears that a single individual is not a species, although for descriptive and collective purposes it may be assumed so to be. When it becomes advisable, which is rarely the case, to make a new species of a single specimen, or when the material upon which it is based is poor and without fruit, there should be something to indicate these facts, as for instance a double dagger as a prefix to the name of the species. This would show that it was simply provisional and possibly not entitled to the rank given it. Just here it may be observed that new species are not so much needed, although they may be occasionally called for, as a better understanding of the limitations of those already in use. It may be further observed that to describe a species by comparing it with another by saying that the leaves in the one are a little longer or a little shorter, a little wider or a little narrower, the leaf cells a little larger or a little smaller, the pedicels a little longer or a little shorter, and this too without giving any measurements, is simply inexcusable. Species so described should not be recognized, neither should the makers of them.

The only way to acquire a true conception of a specific type is to study a number of specimens, the more the better, from as many different localities as possible. Extreme forms, intermediate forms, depauperate forms, all contribute to the series of which the species is the unit of aggregation. Subspecies, varieties, forms, may be necessary, but these will readily fall into their proper places when the specific type is once understood. The dimorphism which every species exhibits, and which is more apparent in some than in others is only to be apprehended by a close study of the intermediate forms. From a taxonomic standpoint the recognition of these dimorphic tendencies is most important, as they are the marks by which the evolution of the type from its lower to its higher forms is shown.

In conclusion allow me to say that it is to be hoped that when this bloodless nomenclatorial war is over and when musty tomes and rotten types have done their worst and when personal aggrandizement has given way to the claims of science, more time will be found for the study of the making and the delimitation ot species. Until that time it is to be feared that many a budding bryologist, full of life and hope, will be nipped by the frosts of many discouragements and driven to other fields in which more satisfactory results await less exacting labors. Rosemont, New Jersey.

HOW TO COLLECT AND STUDY LICHENS.

BRUCE FINK.

Presented at the meeting of the Sullivant Moss Chapter, Philadelphia, Pa., Dec. 31, 1904.

Introductory.

It is a very real pleasure to the writer to be able to contribute to the meeting of the "Moss Chapter" something which he hopes may prove more or less interesting and suggestive. It was his privilege to be present at the meeting at Columbus, where the beginnings of the organization were made, and that meeting was so thoroughly enjoyable and instructive that he feels more keenly the loss at not being able to be at the present one. At the Columbus meeting, he expressed regrets that the lichenists could not have a similar society; but since that time both the bryologists and the mycologists have appeared to be so willing to give us room that we hardly feel the need of any separate organization. Especially safe is it to state that every American worker in lichenology feels grateful to the "Moss Chapter" for opening the pages of THE BRYOLOGIST for our articles on lichens. The work done there is already bearing fruit, and if the present writer can, by sending a paper to this meeting, aid those who have shown an interest in us and our work on the lichens, he will at the same time serve his own ends and those of lichenologists generally quite effectually. So to the matter of collecting and studying lichens without further introductory statement, except to say that only a popular statement can be given in the short time.

Collecting.

Lichens can be collected at any time in the year, but many of them are more likely to show the spore characters better when collected in the fall. They may be collected also on any kind of a day, but more effective work will be done on pleasant days, while many of the minute forms are more easily detected when damp and therefore brighter. The beginner will find